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PHYSICS.—*The measurement of light.*[1]

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INTRODUCTION

I believe that custom allows the annual address of your retiring president to differ from other papers presented before the Society in at least one important respect. Others are expected to bring before you something original in theory, mathematical development, or experimental data. Your president, however, is permitted to choose his own subject without having to convince the Committee on Communications that it will involve an addition to our knowledge of physical science. Taking advantage of this liberty, I have chosen to talk on the subject of the measurement of light, with the intention of discussing problems which will be quite familiar to many of you, and of summarizing established facts rather than attempting to set forth any new ones. My reasons for so doing are two-fold. First, there has recently arisen in the scientific and technical press an unusual amount of discussion of this general subject; and second, some of the published discussions, as well as questions which have come up otherwise, have indicated that there is a certain degree of haziness in our general ideas on the subject.

It is perhaps well to recall also that during the year just past the Society has been favored with two important papers rather closely related to this subject. Dr. Troland in January gave us a carefully reasoned discussion of general principles applying to the study of problems of sensation, which involve physiological and psychological processes as well as physical stimuli. More recently Dr. Gibson has presented the results of his thorough-going experimental determina-

[1] Address of retiring President, Philosophical Society of Washington, January 13, 1923. The text has been modified as made necessary by omission of slides and other illustrative material used in its presentation.

tion of the relative effectiveness of radiant energy in different parts of the spectrum in producing the sensation of brightness under certain conditions. This paper bears directly upon the problems which I wish to discuss later, but first it is desirable to consider some matters of nomenclature and units.

DEFINITIONS AND UNITS

Light.—The word "light" itself is used in many different senses, but for the present purpose I wish to restrict it to a specific one. For this I must perhaps beg the indulgence of some of our members. I know there are some who cherish a little of the ancient sun-worshippers' reverence for light as the mystic agency which stirs into being the processes on which all life depends, and which surrounds us with warmth and beauty. I trust, however, that a prosaic limitation of the term for our present purposes will not lessen anyone's enjoyment of the beauties of the sunset, weigh down the buoyancy which an azure sky gives to our spirits, dim our appreciation of the iridescence which the oil film shows in lowly places, nor blind us to any other of the thousand and one glories which light provides for our enjoyment. If we are to consider light in any quantitative sense we must agree on some precise definition.

The definition which I propose to use may be stated as follows: "Light is radiant energy evaluated in proportion to its ability to stimulate the sense of sight." With respect to the ancient question whether there is then any light when no eye is present to perceive it, this definition is in a way a compromise. The physical radiant energy *is light, but how much light* it is can only be told by applying coefficients depending on the properties of the visual apparatus. According to this definition ultra violet and infra red radiation are therefore not "light." In order to emphasize the similarities of radiations of different wave length it is perhaps allowable to follow Tyndall's example in speaking of "Light, Visible and Invisible," but for precise discussion there are good reasons for adhering to the stricter definition which brings in the coefficient of sensation.

There are in fact many authorities who would go further and say that the term "light" can properly be used only for the sensation aroused by radiant energy, but this can never be a quantitative definition, because we can not measure sensation except in a roughly approximate way. The sensation produced depends on the state of the physiological apparatus involved, so that the amount of sensation produced by radiation of the same kind and amount is widely different

at different times. Moreover, at any given time, sensation is not proportional to the amount of the physical stimulus. In other words, in psychological effects two and two do not make four, and such effects can not well be used as a measure of other quantities. A condition somewhat analogous to this is represented by what the economists call the "law of diminishing returns." For the individual ultimate consumer at a given time two apples are seldom worth twice as much as one, and yet no one would deny that they are twice as many. So in the case of light it would be quite impractical to say that the quantity is doubled only when the sensation is doubled. In actual practice we must have some name which designates the quantity, partly physical, partly physiological and psychological, which represents an amount of radiant energy with due allowance for the usefulness of that particular kind of radiant energy for purposes of vision. The most practical course is to use the term "light" as is done in common speech.

It must be granted, however, that such expressions as "light is emitted from a lamp" or "the light falls on the page" are logically somewhat peculiar, since in these cases the actual phenomena involved are purely radiation, and the visual factor comes in only by a sort of mental juggling. We may perhaps imagine that as the radiation passes from lamp to book some spirit tags each element of it with a coefficient suitable to its frequency, which coefficient represents the magnitude of the effect which will be produced when the radiation reaches the eye. The thing which actually exists in space in definite amounts is radiant energy; the visual factor is obviously not really applied until this energy is absorbed by the visual apparatus and changed into a distinctly different form. In brief, *light in this quantitative sense has no real physical existence.* This fact is an argument against the proposals which have been made to simplify nomenclature by using the same names for units of light as for energy. It has been urged that the unit of light flux be that amount produced by one watt of radiant power at the most effective frequency, and that this unit also be called a watt. As an illustration of the practical confusion which would result from such a nomenclature it may be said that the lamp known as a "60-watt" at present would give about one "watt" of light flux. While this would serve to emphasize how far our practical illuminants fall below the most efficient monochromatic radiation, this advantage would hardly compensate for having two "watts" of entirely different kinds. Since the physiological-psychological sensation factor must come in in the translation of energy

into "light" there is no real disadvantage in passing over to entirely different units at the same time.

Derived Quantities.—If we may then start with this half-imaginary quantity which we call "light" and which is simply energy with the proper kind of tag on it, all the other things with which one has to deal in photometry and illumination follow logically. In nearly all illumination problems we are concerned with the rate at which light is supplied. Letting Q be the amount of light, then $\frac{dQ}{dt}$ = luminous flux (F), that is, the rate of flow of light. In order to deal with this flow mathematically it is frequently necessary to consider three derived quantities, (1) density of flow or flux per unit area $\left(\frac{dF}{dS}\right)$, either from, to, or through a given surface, (2) divergence of flow or flux per unit solid angle $\left(\frac{dF}{d\omega}\right)$, and (3) a combination of these, flux per unit solid angle per unit of projected area $\left(\frac{d^2F}{dSd\omega\cos\vartheta}\right)$. These three latter quantities are, of course, applicable in all kinds of problems, such as that of calculating the flow of light through a projector or other optical system, but more commonly they are used in restricted senses for which special names have been given. The flux per unit area on a surface is its illumination (E), the flux per unit solid angle from a source is its intensity or candlepower (I), the flux per unit solid angle per unit of projected area of a light source is its candlepower per unit of projected area, or brightness. The source concerned need not be a self-luminous one, since the same reasoning will apply to light diffusely reflected or transmitted. Volume sources, such as flames or the sky, have to be considered as virtual surfaces, but this introduces no serious difficulties and is in effect what the eye itself does.

Units.—We have so far talked about these quantities without attempting to tie them up to any definite magnitudes. When we come to the matter of units and actual measurements difficulties arise in following out the logical scheme outlined. It is agreed that the ultimate basis of measurement is to be the effect on the eye, but all that the eye can do quantitatively is to equate brightness. We must somewhere make an arbitrary start in order to establish a system of units. As a matter of history you all know that this has been done by adopting a certain value for the candlepower of some kind of light source, so that historically the candle is the basic unit. From this

followed the foot-candle and the meter candle (also called lux) which, as the names show, were originally defined in terms of candlepower and distance rather than of flux density. The unit of flux itself, the lumen, is in practice defined as the flux through a unit solid angle from a source of unit candlepower, and going farther backward on our scheme the unit of "light" is not even dignified by a distinctive name, but is called the lumen-hour.

TABLE I.—PHOTOMETRIC QUANTITIES

QUANTITY	DERIVATION	SYMBOL	SPECIAL NAMES	UNITS
Light	Energy x V	Q		Lumen-hour
Luminous flux	$\frac{dQ}{dt}$	F		Lumen
Density of flow Flux per unit area	$\frac{dF}{ds}$	E	Illumination (on surface)	Foot-candle Meter-candle Lux
Divergence of flow Flux per unit solid angle	$\frac{dF}{d\omega}$	I	Intensity Candlepower (of source)	Candle
Flux per unit solid angle per unit projected area Candlepower per unit projected area (of source)	$\frac{d^2F}{d\omega\, dS \cos\theta}$	B	Brightness	Candle per sq. in. Candle per cm² (Lambert = $1/\pi$ candle per cm².)

Note.—The nomenclature of this table departs from recognized practice in the use of the term light and the symbol Q for it. The practice approved by the American Engineering Standards Committee is set forth in Illuminating Engineering Nomenclature and Photometric Standards, a pamphlet obtainable from the Illuminating Engineering Society, 29 West 39th Street, New York City.

Brightness is of course most simply specified in candles per square inch or per square centimeter; but another unit, the lambert, has found considerable use. The lambert is $1/\pi$ candle per square centimeter. The reason for using this apparently peculiar unit is most readily explained by an example. If a perfectly diffusing white wall is illuminated it becomes a secondary source of light, but the reflected light is so distributed throughout a hemisphere that the brightness of the wall in candles per unit area is only $1/\pi$ times the illumination. The lambert is the brightness of a perfectly diffusing, completely reflecting surface under a unit illumination, that is one centimeter-candle, which means receiving one lumen per square centimeter. The

lambert may therefore be defined as the brightness of a perfectly diffusing surface emitting or reflecting one lumen per square centimeter. This represents a high degree of brightness for a secondary source. A white surface in the brightest direct sunlight would have a brightness of only 10 lamberts, and for most cases of illuminated surfaces, the millilambert, or 0.001 lambert, is more convenient. The millilambert is the brightness of a perfectly white surface with 10 meter-candles illumination, and since a foot-candle is somewhat over 10 meter-candles (the actual ratio being 10.76), the millilambert represents almost exactly the brightness of an actual white surface, such as magnesium oxide or carbonate, under an illumination of 1 foot-candle.

A special case in which a still smaller unit, the microlambert (one-millionth of a lambert), is used, is the measurement of radio-active self-luminous materials. The brightness of these materials as actually used is a few microlamberts. That is, they are as bright as a white surface receiving a few thousandths of a foot-candle; for comparison, it may be noted that full moonlight is several hundredths of a foot-candle, giving to a white surface a brightness ten times as great as these materials show.

Referring to the table of units we have constructed (Table I), it may be noted that the light output of a source can be specified in two ways. We can either state the flux in lumens or give the average intensity or candlepower over a specified solid angle, usually the complete sphere. From the point of view taken in making up this table it appears that the former practice is simpler, but on the other hand the historical development favors the second, and in other countries than this the flux rating has gained little use in practice. In this country, however, the lumen has rapidly attained an extensive use as a practical unit for two reasons. One is that for rough and ready calculations of illumination the lumen is more convenient. For example, if one has to provide an area of 2000 square feet with an average illumination of 5 foot-candles the product 10,000 immediately gives the net amount of flux required. A more potent reason, however, is the fact that so long as candlepower is considered to indicate light output, misunderstandings are bound to occur, and misrepresentation is facilitated. For instance, when a lamp rated at 21 candles is put in a headlight reflector and gives a candlepower of many thousands, it is not surprising that the non-technical man is puzzled. Even a technically trained man is likely to overlook the fact that watts per candle for one kind of lamp do not mean the

same as for another, and the continual necessity for specifying what kind of candlepower one means is a strong argument for using the unambiguous "lumen." Incidentally the appropriate use of the flux rating which tells how much light is produced serves to make more clear the real significance of candlepower as indicating the distribution of the flux about the source.

STANDARDS OF CANDLEPOWER

Primary Standards.—While all countries agree in making their measurements start from concrete standards of candlepower, and general agreement, with the exception of the Germanic nations, has been reached on a common unit of candlepower, there has been wide divergence in regard to the standards which shall be considered as fundamental. In all countries the name given to the unit is candle, or an equivalent word, but no country has actually retained the candle as a standard. In passing from it, however, each country has followed an independent course, and consequently the nominal basis of the unit is different in each of the four great nations, France, Great Britain, Germany and the United States. The first three of these adopted different flame lamps as primary standards. If the units were actually redetermined from these standards it is probable that differences would appear among the units thus derived.

In France the classic basis for the value of the candle is the Violle platinum standard (the normal intensity of one square centimeter of platinum at the melting point), but no one ever succeeded in reproducing Violle's results, and it is now recognized that this standard is far from being exactly reproducible, because the radiation from platinum is so susceptible to the influence of surface conditions and surroundings. In practice Violle's determination of the unit was applied by using the Carcel lamp with the value which he assigned to it. and until recently this lamp has been retained as a reference standard. A law passed in 1919, however, while still referring to the Violle standard, specifically says that the unit, the Bougie Décimale, is represented practically and in a permanent manner by means of incandescent electric lamps deposited in the Conservatoire Nationale des Arts et Métiers. In Great Britain the candle was superseded by the Harcourt 10-candle pentane lamp, but it is difficult to reproduce such lamps exactly, and the accepted standard is not the pentane lamp in general, but a particular lamp of this kind at the National Physical Laboratory. Moreover, the standards actually used at the National Physical Laboratory are electric incandescent

lamps. These lamps were originally calibrated by comparison with the pentane lamp mentioned, but published records indicate that only two series of such comparisons have been made, so that in effect the unit is maintained by the electric standards. In Germany the Hefner amyl-acetate lamp is presumably still the nominally recognized standard.

In addition to the uncertainties inherent in all these flame standards all of them show systematic variations resulting from atmospheric conditions. Consequently in every case the actual light output of the flame standards under average conditions has been determined by means of incandescent electric lamps, which are free from these effects. The electric lamps have proved so much more reliable and convenient than the flames that there has been a marked tendency to use them as the real standards, comparisons with the flames being rarely made. In fact the international agreement mentioned has been obtained and is maintained by means of comparisons between electric standards initiated by the Bureau of Standards after several attempts made in Europe to reach agreement by direct comparison of flame standards had failed.

This procedure is in part analogous to that followed in the case of the international ohm, which is maintained by means of wire resistance standards whose value is derived from mercury ohms set up at infrequent intervals, but there is the very important difference that there exists no reproducible primary standard of light analogous to the mercury ohm. In the adoption of the unit of candlepower in the United States this difficulty was faced, and it was decided not to adopt any of the existing nominal primary standards, but to base all values on a large group of electric incandescent standards pending agreement on a more satisfactory primary standard.

Some sixteen years have passed since this decision was made. During that time extensive experimental studies of the various flame standards have been made at the Bureau, and the net result of these investigations has been a confirmation of the wisdom of the course taken. Instead of the flame lamps being taken as a fundamental basis for the calibration of the electric standards, this relation is reversed, and the flames are used only as working standards for less precise measurements where facilities are not available for operating electric standards.

For practical purposes the electric standards are highly satisfactory, and the probability of any important drift in the value of the unit maintained by them is almost negligible. Nevertheless it must be

admitted that such a drift is possible. Differences between the various national laboratories may develop and a common primary standard for reference would be highly desirable. Tradition and national pride make it difficult for any country to give up its old standards and especially so as to adopt those of some other nation, but so far as the United States is concerned the field is entirely open, and internationally the time appears favorable for the consideration of new proposals in this field.

In order to receive serious consideration, however, any proposed primary standard of light must be capable of reproducing values with an accuracy better than one per cent, and one-tenth per cent should be within the range of its possibilities. To attain such accuracy will require the most careful application of all our knowledge of radiating bodies, but all recent discussion trends toward defining the unit of light in terms of the established properties of radiators, rather than of trying to devise other special standards of candlepower. The properties of the complete radiator, or black body, are well established both in theory and in experiment, and this line of development appears to hold out most hope for the attainment of a light source exactly reproducible from specifications. The very rapid variation of brightness with change in temperature, which makes optical pyrometry practicable and precise, is, however, a serious and fundamental difficulty in using the black body as a standard of candlepower. At the temperatures which would be most suitable, a change of 1 per cent in temperature makes about 12 per cent change in brightness. Consequently the temperature must be known to about 2 degrees or one-tenth per cent in order to determine the light output to one per cent. The attainment of such accuracy does not, however, appear impossible.

Two comparatively recent experimental investigations bearing directly on the possible establishment of the black body as a primary standard of candlepower have been reported. One of these made by Hyde, Forsythe, and Cady in the Nela Research Laboratory was a determination of the brightness of a carbon-tube furnace over a considerable range of temperatures, including the temperature at which the light from the furnace matched in color that given by the carbon incandescent electric lamps which are now our basic candlepower standards. This temperature was found to be about 2077 degrees K., and at 2077 degrees the brightness was determined as 70.2 candles per square centimeter. This value has therefore been proposed tentatively as an absolute standard of light.

The value of such a specification, however, is obviously dependent on two things which are very difficult to ensure. The first is the establishment of a temperature scale which is reliable and reproducible within 2 degrees or better at these high temperatures, and the second the construction of a furnace which approaches closely enough to uniform black body conditions to make sure that the part of the furnace from which light is emitted is really at the temperature assigned to it.

Now these high temperature scales are based on and checked by melting points of pure metals. Years ago Waidner and Burgess suggested that Violle's idea of using platinum at the melting point for a light standard might be made practicable if the platinum were not used as a free radiating surface, but merely served to control the temperature of a black body. Nearly everybody has recognized the essential soundness of this proposal, but for various reasons it has only recently been tried out experimentally. One of the obvious difficulties was the cost of the platinum which would be necessary if measurements were to be made on a black body radiator of anything like the ordinary kind. A few years ago H. E. Ives conceived the idea of getting the desired effect with a moderate quantity of platinum by using for the black body a hollow platinum wedge electrically heated to the melting point. Observations were made on four wedges with very consistent results, the average brightness found being 58.35 candles per square centimeter, with an extreme range of one-half per cent. Other data obtained in the same investigation were, however, inconsistent, and extensive study would be required to determine the effect of a number of conditions which may affect the results. Although the wedge forms a satisfactory radiator for pyrometric work, there is serious doubt whether it can be satisfactory for the very exact approach to black body conditions required to give reliable candlepower values.

Realizing the difficulties of proving that the wedge form conformed closely enough to complete black body conditions, Ives has more recently studied the possibilities of a hollow cylinder of platinum in which the conditions can be more definitely established. The results of this investigation were reported to the Franklin Institute a few weeks ago, but have not been published.

The preliminary values reported show a highly satisfactory degree of consistency among the observations. Comparison of Ives' results with those of Hyde, Forsythe, and Cady will be somewhat uncertain because it must depend on the temperature to be assigned as the

melting point of platinum on the Nela Research Laboratory scale. It would appear, however, that agreement of the photometric results could be obtained by assigning such a temperature well within the range of the present uncertainty of the true melting point.

While more refinement will be necessary before the practical standards of candlepower can be based on black body measurements, these two investigations seem to justify hope that this can eventually be done so that we shall have a really reproducible primary standard depending only on the melting point of platinum and the complete radiator, and otherwise independent of the properties of materials.

Flux Standards.—Before passing from the subject of standards it may be well to revert to the question of measurement of flux. All the standards mentioned serve to furnish values only of candlepower or brightness, whereas now-a-days all lamps are preferably rated in lumens, so that some sort of transformation of values is necessary. This is accomplished in two steps. Standards of flux (or of mean spherical candlepower) are established by measuring successively the candlepowers of electric lamps at various angles from the axis of the lamps. The total flux can then be calculated, and these lamps are then used as standards in an integrating sphere for the measurement of other lamps.

A full discussion of the theory of the sphere as actually used is hardly practicable here, but the basic principle is exceedingly simple. The interior of the sphere is supposed to have a perfectly diffusing surface; every element of this surface when illuminated reflects light of an intensity which is at a maximum normal to the surface and falls off in proportion to the cosine of the angle as one departs from the normal. If we consider where this light falls on another element of the surface, we find by a simple calculation that each element of the surface illuminates every other one equally. The result is that, if we do not count in the original light which fell on the surface to make it luminous the illumination of all parts of the interior is the same. Furthermore if the wall is uniform, the illumination of all parts will be the same no matter how the original light was distributed. If we therefore put a lamp in the sphere, add a small screen to shade a spot from the lamp, and then measure the brightness of that spot, we have a measure of the total flux from the lamp.

In actual practice the theoretical requirements of the sphere can be approximated so closely that the systematic errors can be kept below one per cent except in extreme conditions. All lamp factories and important laboratories now use spheres, and a large proportion of the standard lamps called for are flux standards.

THE FUNDAMENTAL QUESTION OF LIGHT EVALUATION

Limited Significance of Measurements.—It may be considered that we have for practical purposes fairly complete and satisfactory paraphernalia for light measurements. It should be noted, however, that so far nothing has been said about the properties of the eye which was, by the definition of light, made the arbiter as to what is "light" and what is not. Most of the measurements that have been mentioned might be made by a bolometer or other radiometer as well as by the eye if it were found convenient to do so, since it has been tacitly assumed that the lights to be compared would be alike in quality. So far, therefore, we have dealt with definite physical relations wherein the attainment of accurate values is a straightforward problem of laboratory technique. Personal and instrumental errors of course, come in, but in each case there is a definite and correct answer to the problem, and this correct result can be approximated more and more closely in proportion to the skill and patience shown by the observers. Unfortunately, however, this condition does not apply to many of the so-called "measurements" of light which we have to make.

I began by defining light as radiant energy evaluated in proportion to its ability to stimulate the sense of sight, but we must ask what "sight" means when we wish to compare lights that differ distinctly in spectral composition. Does it mean ability to see fine details distinctly, to distinguish differences of light and shade, or does it mean the ability to take in a general perception of our surroundings? Of course, sight means all of these combined, but unfortunately for our "measurements" of light, when we have to deal with lights of different quality these different functions of sight assign different relative values to them, and in order to get any start at all on quantitative data we shall have to say more exactly what we mean by the sense of sight.

So long as the lights dealt with are of about the same color it is easy to find the relation between them, and it has been generally agreed that measurements shall be made by some device which obtains an equality of brightness on two white surfaces side by side. As a natural development the same procedure has commonly been used for lights differing in color, and following this custom we may assume "sense of sight" for the present purpose to mean "sense of brightness." But when we have to deal with lights of considerable color difference it becomes doubtful what is meant by equal "brightness." The

relative subjective brightness of two different colored surfaces may vary considerably according to the part of the retina the light falls on, for most parts of it the relative brightness depends more or less on the absolute brightness, and in general it is different for different people.

As a convenient illustration of these variations a piece of blue paper on a red background may look brighter than the red at low illuminations, become equally bright at somewhat higher illumination and decidedly darker at a still higher level. The transition point depends on the size of the samples used, and different persons will place it at quite different illuminations. Moreover, when the illumination is fairly high and the color of the surfaces distinctly different, most persons find it almost impossible to judge when equal brightness is attained.

The question is then how shall we determine when colored lights are equally bright, or how otherwise shall we meet the need of describing these lights in definite terms. To begin with, it should be recognized that the thing we want to define and measure has no real and definite existence, but must be more or less arbitrarily established. The problem is not merely one of finding a value for a quantity which varies with other conditions. If one unit of radiant energy gives rise to a certain sensation, we must remember not merely that ten times as much energy does not produce ten times the original sensation, but that the multiplying factor may vary appreciably with conditions. An analogy may make the troubles more evident. It is sometimes necessary to compare a length standard of brass with one of platinum. In order to have a definite ratio they must, of course, both be kept at specified temperatures. Now let us imagine three kinds of difficulties to arise; first, when the meter of brass has been carefully adjusted to equal the meter of platinum, a measurement by centimeters indicates that one bar is longer than the other; second, if we change the size of the microscope field used for observations one bar expands and the other contracts; third, each individual observer when he looks through the microscopes causes a temporary differential shrinkage or expansion of the two bars, the amount of this differential being more or less characteristic of the individual, but varying somewhat from day to day. Now if all these troubles really arose I think we would be inclined to say that comparing brass meters with platinum was a useless undertaking. Yet when we make such a measurement as the direct comparison of a normal tungsten filament lamp with a carbon lamp there are systematic difficulties closely analogous

to all three mentioned, with the added one that for many would-be observers there is great difficulty in deciding on any values at all. Moreover the magnitude of the effects concerned is not, in general, negligible in comparison with the accuracy expected in practical measurements.

I think I have enlarged on this point sufficiently to make clear what I wish to emphasize, namely, that the ratio of the subjective brightness of two lights of different color is not a definite and single-valued quantity, but depends upon the conditions of observation as well as upon the characteristics of the individual observer's eye. In particular, in order to make values definite it is necessary to specify more or less precisely the size and brightness of the photometric field, which determine the parts of the retina used and its level of adaptation. It is also necessary to include observations by a sufficient number of individuals to approximate the results which would be obtained by an imaginary average, or normal, observer. It is obvious also that relative values determined under these limited conditions must not be applied indiscriminately. For example, a comparison of two lights under the standard conditions may not show at all accurately their relative effectiveness at the low illuminations often used on highways, nor does a measurement of brightness necessarily show accurately the relative merits for work on fine details.

Nevertheless it is useful and almost necessary to have some means of describing in a quantitative and comparable way the different kinds of light we have occasion to use. We actually are measuring them continually and I shall try to outline briefly the present status of methods for such measurements.

Direct Equality-of-Brightness Observations.—The earlier illuminants were nearly alike in color. Consequently the complications just described did not become practically important until recent years. By the time these problems arose, excellent instruments had been developed and photometric laboratory apparatus was more or less standardized throughout the world. The natural course, therefore, was to use these instruments and methods and to do the best one could with them in measuring the newer illuminants. The rapid development in succession of metallized carbon, tantalum, vacuum tungsten and gas-filled tungsten lamps has accentuated the difficulties and has made them affect the most common types of commercial photometric work. Other developments, such as mercury vapor lamps, nitrogen discharge tubes, and neon lamps, as well as special filters and glasses to give color effects, have created still more serious difficulties for the photometric laboratory.

Because of the difficulty of obtaining reliable and reproducible results, the practice early developed of preserving values on certain steps by the calibration of color filters, such as blue glasses, which, interposed on one side of the photometer, would equalize the color of two different lamps. Another procedure has been to make many observations to determine values for secondary or derived standards of the different kinds of lamps to be measured.

The extreme case of the latter procedure is perhaps that followed by the British National Physical Laboratory where six sets of electric standards were carefully calibrated by equality-of-brightness observations covering the range from the color of the pentane flame standard up to a low-efficiency vacuum tungsten lamp. This was very carefully done by a number of experienced observers and affords a good example of the essential uncertainty of the procedure. The divergence between different observers, of course, increased on the successive steps, and a difference of nearly 3 per cent in the final values developed between extreme observers. Measurements were also made in which the same observers jumped directly from the lowest to the highest efficiency within this range. The average candlepower of the higher efficiency lamps by the two methods agreed within 0.3 per cent, but it is significant that in the direct single step with larger color difference, the observers were grouped more closely than on the final results obtained step by step. The total range of the results and the average deviation from the mean in the direct measurement are only six-tenths as great as in the step-by-step or cascade measurements. When it is considered that the range of efficiencies covered is only from 2 to 6.5 lumens per watt, whereas present day gas-filled tungsten lamps fall considerable above 20 lumens per watt on the same scale, it is evident that the results are not satisfactory for precise measurements in which an accuracy better than 1 per cent is expected.

At the Bureau of Standards a somewhat different course was followed in that instead of establishing different sets of standards for different efficiencies, values for a group of tungsten lamps were determined over a considerable range of efficiencies and the variation expressed in curves and equations. Furthermore, this work developed the fact that for all the ordinary sizes of vacuum tungsten lamps the same equations would apply. In other words, the variation in candlepower and efficiency as a vacuum tungsten lamp is changed in voltage is a definite property, presumably because it represents in a fairly simple way the properties of radiating tungsten at given

temperatures in a vacuum. These equations, known as the Middlekauff-Skogland equations, have been very useful and still constitute the best established procedure for determining relative values for vacuum tungsten lamps at various efficiencies. Moreover, the experience gained at the Bureau since these equations were established indicates that the values were very fortunately chosen. They apparently agree well with the National Physical Laboratory results over the range where they overlap, and the values have been rather closely checked by successive groups of observers at the Bureau during the undesirably rapid change of personnel which we have suffered during the last few years. It should be emphasized, however, that groups of observers of at least equal experience in other laboratories have not agreed so well with these results.

Some years ago very careful comparative measurements on lamps and glasses representing the step from carbon lamps to tungsten were made by groups of five or six observers in each of the three laboratories in the country best fitted for this work. Measurements made a number of months apart indicated that each laboratory adhered very closely to its average result, the consistency of performance in this respect being within one-quarter of one per cent. However, the difference between two of the laboratories became as large as 1.9 per cent, although the range covered was only that from carbon lamps to vacuum tungsten and did not approach the color of gas-filled lamps. The Bureau of Standards was at one extreme in the results obtained, but other data have been in general more consistent with the Bureau's own measurements in this intercomparison, and consequently these values as represented in the Middlekauff-Skogland curves have been retained. The rather imperfect agreement obtained in the intercomparisons has been accepted as a practical corroboration of these values, but again it is somewhat disturbing to have differences of the order of 2 per cent in checking values which are supposed to be maintained to a fraction of 1 per cent.

The result of such comparisons as have been referred to has been to establish agreement on more or less arbitrary values for standards of successively higher and higher temperature and efficiency, which, when once established, can be maintained as practically independent standards for future use. In fact in the case of the National Physical Laboratory it is definitely stated that the values of the higher efficiency standards are "assigned to them once for all." Such multiplication of standards, or determination of curves, while serving to meet the more urgent commercial needs, is limited in its application.

There is always doubt as to the certainty with which the values can be reestablished by reference to the fundamental standards even though the color differences involved are the relatively small ones represented by incandescent lamps of different efficiencies. Furthermore, this method is, of course, not at all applicable to color differences which can not be reached step by step or for which there is no suitable secondary reference standard available.

Flicker Photometry.—Various methods for meeting or avoiding these difficulties have been proposed. The only direct method which has given any evidence of practicability is the use of the flicker photometer. This type of photometer differs from the ordinary one in that it presents the two photometric surfaces to view alternately in the same place instead of simultaneously side by side. One of the peculiarities of vision is the fact that the two colored surfaces can be interchanged at such a rate that the disturbing color differences are no longer seen, although differences of brightness still show in the form of a perceptible flickering of the light in the field. Consequently a speed of alternation can be chosen to get rid of the color sense, and then a setting for equal brightness can be made by finding the point at which the flicker disappears or reaches a minimum. Whether the "brightness" thus found is exactly the same thing as that found by the ordinary photometer is a disputed question. As we have already seen, however, brightness must be rather arbitrarily defined in either case, and it appears that a suitable choice of conditions will give a very close agreement between the two methods.

The results obtained by the flicker photometer are not free from variation with conditions such as field size, absolute brightness, and individual characteristics of the observer. The advantages of the instrument are simply that definite results can be obtained on any color difference by any observer and that the results at various times and on related color differences are far more consistent than when measurements are made by the ordinary photometer. This consistency makes it possible to establish the characteristics of an observer and to predict rather accurately what results he will obtain in comparison with an average observer. As an example of the possibilities in this kind of procedure, the Bureau of Standards has for a considerable time made use of a simple test originally due to Ives which indicates the relative values an observer may be expected to obtain when comparing lights of certain types. The test measurement is the determination of the transmission of two solutions, one of which transmits the blue end of the spectrum while the other transmits the yellow end.

The densities of the two solutions are so chosen that for the average observer they transmit equal percentages of light from the standard carbon lamp. In general, different observers find different values for the transmission of each of these solutions under standard conditions, and it is convenient to use the ratio of the two transmissions as a rough indication of the characteristics of the observer. The use of such tests is not merely a matter of finding "normal" and "abnormal" observers, for we find that the characteristics of observers extend over a considerable range with a distribution roughly approximating that represented by the ordinary "curve of errors." It should be noted that this distribution does not represent error of measurement since each observer will repeat his values with a certainty represented by a very small part of the range of distribution. In other words, the curve represents the real distribution of individual characteristics, not the errors in determining those characteristics.

This characteristic ratio for a given observer is closely correlated with the results which the observer will obtain in comparing the light emitted by radiators at different temperatures, such as lamps of different efficiency. It is therefore possible by the use of these test measurements to reduce the results obtained by any small group of observers to a normal value with a considerable degree of certainty. An illustration of such correction is shown in Table II which represents an actual case where five observers determined a ratio equivalent in color to the step from vacuum tungsten to gas-filled standards.

It happens that at the time this work was undertaken the group of observers available showed a wide variation in test ratios as given in column 2. Consequently their deviations from the average were very large, as shown in column 4. Nevertheless when corrected they gave an agreement which is practically perfect for such work, the average deviation of the individual observers being less than one-tenth of one per cent from the mean. In this particular case it happens that the observers are so distributed that their uncorrected mean is equally good, but this condition can not be depended upon. Ordinarily a group of observers will show much less scattering in their characteristics so that the individual corrections will be less important than they were here, but one can not depend upon a small group being symmetrically placed with reference to the mean as this one was.

It should perhaps be noted that for relatively small color differences the flicker photometer is not as precise as the ordinary form. For the moderate color differences represented by the step from carbon

to tungsten lamps, the advantage of the flicker lies not so much in greater precision as in the certainty that the results obtained in a particular case are consistent with those obtained at other times and that they will agree with results made by similar methods by a different group or a different laboratory. In other words, the variations shown by the flicker method are scattered about a definite point in a purely accidental fashion, whereas the measurements by equality-of-brightness by an experienced observer may be just as consistent among themselves and yet be greatly influenced by a permanent prejudice.

The test-ratio scheme here illustrated is of course of limited use. Nevertheless it makes possible a decided improvement in the certainty of the measurements which are now of most practical importance, and the general principle of test measurements and of establishing normal values which will be accurately reproducible is

TABLE II.—Correction of Individual Observers' Results to Normal

Color difference equivalent to step from 9.3 to 18 lumens per watt

Observer number	Test ratio	Deviation from normal	Photometer ratio	
			Observed	Corrected
1	0.900	−0.087	0.2625	0.2648
2	0.909	−0.079	25	46
3	0.981	−0.006	47	49
4	1.024	+0.037	59	49
5	1.110	+0.123	86	54
Mean.....	0.985		0.2648	0.2649

applicable to all sorts of measurements. It must be admitted, however, that the principle of the flicker photometer appears to be much farther removed from the actual conditions under which light is used than the simpler photometers are. The general adoption of this method of comparison is therefore doubtful.

Visibility Curves.—In discussing the definition of light it was suggested that we might consider each element of radiant energy to be tagged with a coefficient representing its relative visual effect. Further consideration has shown that the value of this coefficient is somewhat indefinite. Nevertheless if we agree on definite conditions and methods of observation, an average value can be given for this coefficient for each wave-length. A complete solution of the problem of photometry must include the determination of these relative visual coefficients, and when they are established all other comparisons might be based on them. It is, however, no simple matter to establish

satisfactory values for these coefficients or for the "visibility" curve which represents them. Fortunately it is only a few weeks since Dr. Gibson presented to the Society the general results of the most recent work on visibility curves, and consequently I shall not burden you with details of the methods of determining them. While Gibson and Tyndall's curves were determined by equality-of-brightness settings, following a step-by-step method used earlier by Hyde, Forsythe, and Cady, the greater number of such determinations have been made by use of the flicker principle. The most extensive investigation of this kind was carried out by Dr. Coblentz a few years ago, and was likewise reported here.

If one plots together the curves obtained for individual observers in any of these investigations the result is a diffuse band representing the scattering of the curves of different individuals. While there is a general similarity in the curves of all observers, individual differences are such as to represent a very large percentage variation. Here, as in the case of the simpler test-solution measurements already mentioned, any classification of observers as "normal" or "abnormal" must be arbitrary, since a complete gradation is found between the most extreme types of curves. The choice of a general average or "normal" curve is therefore difficult.

The results of Gibson and Tyndall and of Hyde, Forsythe, and Cady, both made by equality-of-brightness methods, show an agreement which is really remarkable when the difficulties of the investigation are considered. In fact it is quite possible that the differences found are to be explained by the fact that observing fields of different size and form were used in the two laboratories.

Gibson's comparisons of his own results with those of Coblentz also show a fairly close agreement, but apparently indicate a real difference between the results of the two methods. There are two conditions, however, which make this conclusion somewhat uncertain. These are that the energy values used were determined in entirely different ways, and that several years elapsed between the two investigations so that the observers common to both may have changed their characteristics in the meantime. The conditions of field size and brightness used were also slightly different.

My opinion is that most of the earlier determinations of visibility may now be given little weight, and that the most reasonable method of establishing experimentally a reliable normal, or average, curve is to carry out such measurements as are necessary to answer the unsettled questions with regard to the differences between the Coblentz

flicker data and the results of the other determinations mentioned. All of these were carried out with such care and thoroughness that they should serve to establish a standard curve which would approximate as closely as can be expected to the characteristics of the hypothetical average observer.

When such a standard visibility curve is established, the total light value of radiant energy of any quality for which spectral distribution curves can be obtained will become merely a matter of calculation or of graphic determination. It is evident, however, that these visibility curves are subject to all the limitations of brightness measurements. Using them, consequently, does not solve all our difficulties. Moreover, the experimental difficulties in obtaining reliable spectral distribution curves are considerable, and the aggregate errors of calculated values may often exceed the uncertainty in the direct comparison by simpler methods. Values calculated from spectral measurements and visibility curves are in fact hardly capable of an accuracy such as is most commonly desired in photometric results.

In this connection mention should be made of two interesting possibilities in the way of more reliable comparisons by means of visibility curves, both of which have been described in recent years before the Society. One is the combination of Nicol prisms and quartz plates, devised by Priest, which constitutes a color filter whose spectral transmission curve is adjustable over considerable limits and can be calculated with a high degree of certainty. These calculations as expressed in terms of light must, of course, depend on the visibility curve, but having a standard visibility curve, the results obtained by this device should be reliable.

The other possibility is that of a physical photometer consisting of an instrument for measuring radiant energy, combined with a filter which in effect adjusts the spectral sensibility of the radiometer to make it correspond to that of the standard curve adopted as representing the sensibility of the eye. The refinement of laboratory technique required to make accurate measurements of radiant energy is such that this method of measurement does not appear likely to find very extensive application, but its possibilities may be worth further investigation.

CONCLUSION

While applications of the visibility curve will undoubtedly constitute a valuable supplement to more direct measurements, the situation will never be entirely satisfactory unless the values thus calcu-

lated can be checked by more direct measurements. It is, therefore, highly desirable that when a standard visibility curve is adopted it be made to fulfill this condition. In order to do so it may possibly be necessary to adjust the experimentally determined visibility curve to a slight extent. Whether this will be necessary or not can only be determined by comparing results calculated from various visibility curves with those established by direct measurement. As has been repeatedly indicated above, results obtained by all methods depend to some extent on conditions which have to be chosen more or less arbitrarily. It is to be hoped that agreement can be reached on the specification of conditions such that consistent results can be obtained whether one uses the equality-of-brightness photometer, the flicker photometer, or spectral measurements in combination with a visibility curve. There appears to be no doubt that conditions can be so specified as to bring about this condition so far as comparison of incandescent radiators at different temperatures is concerned. For instance, if the standard visibility curve be adjusted to make it agree with the usual equality-of-brightness photometer as now used for these measurements, flicker values can readily be brought into agreement by correcting them to a "normal" slightly different from the results which that instrument would give when used by the average observer. There would appear to be some justification for adopting as one of the basic conditions the use of the small central portion of the retina which is actually used in observation of details. If this were done it is probable that all three of the photometric methods mentioned could be brought into very close agreement.

If such a solution for the fundamental problem of comparison of brightness can be obtained, and the black body is made to furnish a standard reference source, our system of assigning values to lights of every kind will probably be in as satisfactory a condition as the intrinsic difficulties of the subject will ever permit.

PROCEEDINGS OF THE ACADEMY AND AFFILIATED SOCIETIES

PHILOSOPHICAL SOCIETY

874TH MEETING

The 874th meeting was held in the Cosmos Club Auditorium on Saturday, December 16, 1922. It was called to order at 8:20 P.M. by President WHITE with 33 persons present.

The minutes of the 872d meeting were read and approved.

By invitation, Mr. A. H. BENNETT addressed the Society on *The aberrations of anastigmatic photographic lenses.* The address was illustrated by lantern slides, and was discussed by Messrs. WHITE, HEYL, WILLIAMSON, LAMBERT, PAWLING, L. H. ADAMS, and PRIEST.

Author's Abstract: In the design of photographic lenses, two requirements are dealt with which are not often found combined in other types of optical design.

In the first place, the photographic lens must have a wide angular field of view, which commonly ranges from 40° to 60°. In addition, a large relative aperture is necessary in order that the lens should have sufficient rapidity. These two requirements prohibit an extremely fine correction for the aberrations.

The definitions, general effects on the defining power of the lens, and suitable method of plotting graphs of the aberrrations, together with methods of measurement were given. Average values for the magnitudes of the different aberrations as found from a series of measurements on twenty-five (25) lenses made by eight manufacturers, were presented.

Improvement in this type of lens by the employment of aspherical surfaces, which are more favorable for the correction of the aberrations, will probably result in a great increase in relative aperture, without increasing the aberrations beyond their present values.

Mr. S. P. FERGUSSON and Mr. R. N. COVERT presented a paper on *The measurement of the wind*, which was read by Mr. FERGUSSON. The address was illustrated by lantern slides, and various types of anemometers were exhibited. The paper was discussed by Messrs. WHITE, PRIEST, L. J. BRIGGS, HECK, and HARPER.

Author's Abstract: A review of the progress of anemometry during the past century shows very clearly that much of the confusion or discordance in data of the direction and velocity of the wind is due to the absence of definitions. It is not necessarily difficult to rate an anemometer with a precision satisfactory for most uses, but, if there is considerable difference in the frequency of readings the indications of two similar instruments may disagree as much as 50 per cent and both be correct. The cause of this is the extreme variability of the wind, of which the direct fluctuations may extend 50 per cent or more on either side of the average velocity prevailing during a period of time exceeding one minute; ten or more oscillations may occur in one second. The extreme range of mean velocity, or the movement of the wind during periods of five minutes or longer (published in current reports), extends from a calm to about 60 meters a second, the latter velocity occurring sometimes at exposed stations. At most inland stations, however, the extreme velocity of gales seldom exceeds 30 meters a second. Another characteristic of the wind, of considerable importance in the design of ane-

mometers or structures that must resist high winds, is the small extent of gusts or extreme variations in velocity; momentary differences of 30 per cent have been observed between well-exposed anemometers less than two meters apart, while long-period variations are usually nearly synchronous over areas whose width or extent may exceed 100 meters. Obviously, therefore, two standards are needed, one for average velocities and one for gustiness, the measurement of which can best be accomplished by the use of separate instruments of appropriate sensitiveness.

The factors or constants of most anemometers in general use were determined with fair accuracy by means of whirling machines, at low and moderate velocities, about thirty years ago; but, only within the past few years has it been possible to rate an instrument at the high velocities sometimes attained by the natural wind. The modern wind-tunnel, designed particularly for experimental studies in aerodynamics, has proved to be a most excellent device for standardizing anemometers, as shown by the recent work of BRAZIER in France, PATTERSON in Canada, and that of the Weather Bureau. The Bureau of Standards, having kindly placed the wind-tunnels of its aerodynamical laboratory at the service of the Weather Bureau, the authors, beginning in March, 1922, have tested about thirty anemometers of various patterns, dimensions, and weights at velocities throughout the range of the natural wind, and at various angles of inclination. In August, 1922, fifteen of the same instruments were taken to Mount Washington, New Hampshire, for comparison in the high natural wind prevailing there in order to ascertain differences of behavior in steady and variable winds. The purpose of this work is to determine corrections for the present standard anemometer and to develop a new standard indicating true velocities, for routine and special uses. It is expected that this investigation will be completed during the coming year. The following preliminary results, subject to revision, are considered important:

Velocities indicated by the small Robinson anemometer in use in the United States are approximately 22 per cent too high. The factor determined in wind-tunnels appears to be more nearly constant than that previously ascertained by means of whirling machines.

The factors of many anemometers in use have been determined at velocities throughout the range of the natural wind.

The differences between instruments in the wind-tunnels and in the natural wind appear to be small; not much larger than the normal differences between anemometers of the same kind.

The differences between average velocities indicated by light and heavy anemometers compared on Mount Washington are small; none larger than 3 per cent have been found, so far, and the heavy Robinson instruments tend to under-register. This experiment may require repetition since the results stated are not strictly in accord with earlier work.

The rate of an anemometer increases as the axis is inclined, reaching a maximum with an inclination of about 30°. This was discovered by BRAZIER and confirmed by the experiments at the Bureau of Standards.

The three-cup type of ROBINSON anemometer suggested by PATTERSON appears to be more satisfactory than the usual four-cup instrument. Its factor is more nearly constant and, since but one cup at a time is sheltered, three cups are practically as effective as four of the same size.

It is hoped that these independent investigations in anemometry will result in the adoption of the standards of velocity and methods of measurement referred to and the determination of factors whereby the rate of any anemometer can be ascertained when its dimensions and weight are known.

Dr. L. J. BRIGGS presented an informal communication on *Direct measurement of air speed in wind-tunnels.*

Author's Abstract: At the request of the National Advisory Committee for Aeronautics, the Aerodynamical Physics Section of the Bureau of Standards has made some direct determinations of the air speed in a wind-tunnel for comparison with pitot-tube measurements of air speed.

The method employed was as follows:

A small parallel pencil of light from an arc source was so reflected that it traversed the tunnel three times at right-angles to the longitudinal axis of the tunnel and was finally focused upon a photographic film carried on the surface of a rapidly rotating drum. The distance between adjacent beams measured along the axis of the tunnel was approximately one meter.

Small balloons filled with hydrogen and weighted so as to have the same average density as the air-stream were carried through the tunnel with the air-stream. Each balloon eclipsed successively the three light-beams and the instant of the eclipse was indicated by a break in the trace on the photographic film. Timing lines, representing time intervals of one-thousandth of a second, were simultaneously recorded on the film with the aid of a 500 cycle tuning-fork operated by an electron-tube drive. By this means the time required for the balloon to travel from one light-beam to the next could be measured, which, in connection with the known distance between the light-beams, gave the data necessary to determine the air-speed. The pitot-tube pressure developed by the air-stream was also continuously recorded on the film with the aid of a diaphragm gauge equipped with a mirror. Preliminary measurements indicate that the mean of the speed determinations by the balloon method agrees with the air-speed measurement as determined by the standard pitot-tube to within two-tenths of one per cent.

Adjournment at 9:55 P.M. was followed by a social hour.

875TH MEETING

The 875th meeting was a joint meeting with the Washington Academy of Sciences in the Cosmos Club Auditorium on Thursday, December 21, 1922. The meeting was addressed by Dr. H. A. CLARK of the Taylor Instrument Company of Rochester, New York, on *The manufacture of thermometers.* The address was illustrated by lantern slides, and some specimens of thermometer tubing and of thermometers were exhibited. The paper was discussed by Messrs. HUMPHREYS, JONES, KADEL, and HAWKSWORTH. The meeting adjourned at 9:40 P.M.

876TH MEETING

The 876th meeting was held in the Cosmos Club Auditorium January 13, 1923. It was called to order at 8:30 P.M. by President WHITE, and 62 persons were in attendance.

The address of the evening was given by the retiring President, E. C. CRITTENDEN, on *The measurement of light.* The address was discussed by Messrs. PAWLING, L. H. ADAMS, HAWKSWORTH, WILLIAMSON, and MOHLER. It will be published in an early number of the Journal of the Washington Academy of Sciences.

Adjournment at 10 P.M. was followed by a social hour.

J. P. AULT, *Recording Secretary.*

SCIENTIFIC NOTES AND NEWS

The third paper of the series discussing the Quantum Theory was presented before the Physics Club, Bureau of Standards, by F. L. MOHLER, January 22, 1923. The subject was *The atomic structure of chemical elements.* An abstract by the author follows:

We assume that a chemical element of atomic number Z contains Z electrons each in an orbit determined by quantum conditions similar to those demonstrated experimentally for hydrogen and helium. A rigid mathemattical solution is impossible but in a heavy atom the inner X-ray orbits approximate hydrogen orbits around a charge Z. An analysis of X-ray spectra shows that all positions of equilibrium within the atom are occupied by electrons. There are one K, three L, five M, seven N, and five O levels known. The total quantum numbers are K_1, L_2, M_3, etc. The number of electrons within each orbit can be estimated and the ellipticity of orbits is known. There are as many shapes as allowed by the total quantum number. It is concluded that both in the formation of an atom and in the course of the periodic table groups K, L, M, etc., appear in the order of their quantum number and within each group it is the most elliptical orbit which appears first.

The space configuration must be symmetrical in a complete group and by non-mathematical reasoning we see that the rare gases mark stages in completion of groups and that chemical and physical properties of successive elements can be predicted in some detail from the scheme of atom building indicated by X-ray spectrum theory.

Dr. L. B. TUCKERMAN delivered the fourth lecture of the above series on January 29, 1923. His subject was *Continuity vs. discontinuity.*

Since the beginning of human thinking there have been two different ways of looking at the physical universe. From the one viewpoint the apparent discontinuities of physical phenomena are only singularities in an underlying continuous substratum. From the other viewpoint the apparent continuities of physical phenomena are only statistical averages over underlying discontinuities. Nearly twenty-five hundred years ago Zeno of Elea in his paradoxes pointed out the difficulties involved in both these conceptions.

After the introduction of the calculus as the basic mathematical tool of theoretical physics, the viewpoint of continuity grew to be the customary viewpoint of the physicist. The critical investigation into the mathematical idea of continuity by Bolzano, Weierstrass, Cantor, and others, together with the growth of the modern atomic theory in the hands of Dalton and his followers, the introduction of the electron by J. J. Thomson and finally the quantum by Planck, have rendered this viewpoint less satisfactory.

The development of a theory which shall weld these discontinuities into a coherent whole with the continuities of the older physics is a leading problem of physics today. It is too early to be certain in which direction this development will take place. It may be that the electron, the atom and the quantum will finally be interpreted in terms of an underlying continuity. On the other hand it may be (and the development of the Bohr atomic model seems to make this more probable) that our future picture of the physical world will be essentially discontinuous in all its elements.

The American Nature Association has published the first number of its new monthly periodical, "Nature Magazine." This publication provides accurate and readable information on outdoor subjects and presents the material in an entertaining fashion. All information can be obtained from the American Nature Association, 1214 Sixteenth Street, Washington, D. C. The editors are P. S. RIDSDALE and A. N. PACK.

The Maryland-Virginia-District of Columbia branch of the Mathematical Association of America held a semiannual meeting at the Bureau of Standards, December 9, 1922.

Dr. WILLIAM N. BERG has resigned his position as pathological chemist in the Pathological Division, Bureau of Animal Industry. He is now engaged in the manufacture of biological products at the Berg Biological Laboratory, Brooklyn, New York.

Mr. C. A. BRIGGS, associate physicist, Division of Weights and Measures, Bureau of Standards, has been transferred to the U. S. Department of Agriculture to the post of live stock weight supervisor in the Packers and Stockyards Administration.

Mr. M. R. CAMPBELL, geologist in charge of the coal section of the U. S. Geological Survey for the past 16 years, has been relieved of this duty at his own request that he may devote himself to the physiographic work of the Survey. Mr. W. TAYLOR THOM, JR., has been assigned to succeed Mr. Campbell in the coal work.

Dr. J. C. KARCHER of the Sound Laboratory, Bureau of Standards, has resigned to accept a position as technical adviser to the production manager of the Western Electric Company, Chicago, Illinois.

Mr. W. P. WOODRING has been granted leave of absence from the U. S. Geological Survey to do private work in South America.